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NEO-DARWINISM AND NEO-LAMARCKISM.

By LESTER F. WARD.

Annual address of the President of the Biological Society of Washington delivered Jan. 24, 1891. A historical and critical review of modern scientific thought relative to heredity, and especially to the problem of the transmission of acquired characters. The following are the several heads involved in the discussion Status of the Problem, Lamarckism. Darwinism, Acquired Characters, Theories of Heredity, Views of Mr. Galton, Teachings of Professor Weismann, A Critique of Weismann, Neo-Darwinism, Neo-Lamarckism, the American "School," Application to the Human Race. In so far as views are expressed they are in the main in line with the general current of American thought, and opposed to the extreme doctrine of the non-transmissibility of acquired characters.

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What is the Problem?

In seeking a means of protection from lightning-discharges, we have in view two objects,—the one the prevention of damage to buildings, and the other the prevention of injury to life. In order to destroy a building in whole or in part, it is necessary that work should be done; that is, as physicists express it, energy is required. Just before the lightning-discharge takes place, the energy capable of doing the damage which we seek to prevent exists in the column of air extending from the cloud to the earth in some form that makes it capable of appearing as what we call electricity. We will therefore call it electrical energy. What this electrical energy is, it is not necessary for us to consider in this place; but that it exists there can be no doubt, as it manifests itself in the destruction of buildings. The problem that we have to deal with, therefore, is the conversion of this energy into some other form, and the accomplishment of this in such a way as shall result in the least injury to property and life.

Why Have the Old Rods Failed?

When lightning-rods were first proposed, the science of energetics was entirely undeveloped; that is to say, in the middle of the last century scientific men had not come to recognize the fact that the different forms of energy—heat, electricity, mechanical power, etc.—were convertible one into the other, and that each could produce just so much of each of the other forms, and no more. The doctrine of the conservation and correlation of energy was first clearly worked out in the early part of this century. There were, however, some facts known in regard to electricity a hundred and forty years ago; and among these were the attracting power of points for an electric spark, and the conducting power of metals. Lightning-rods were therefore introduced with the idea that the electricity existing in the lightning-discharge could be conveyed around the building which it was proposed to protect, and that the building would thus be saved.

The question as to dissipation of the energy involved was entirely ignored, naturally; and from that time to this, in spite of the best endeavors of those interested, lightning-rods constructed in accordance with Franklin's principle have not furnished satisfactory protection. The reason for this is apparent when it is considered that the electrical energy existing in the atmosphere before the discharge, or, more exactly, in the column of dielectric from the cloud to the earth, above referred to, reaches its maximum value on the surface of the conductors that chance to be within the column of dielectric; so that the greatest display of energy will be on the surface of the very lightning-rods that were meant to protect, and damage results, as so often proves to be the case.

It will be understood, of course, that this display of energy on the surface of the old lightning-rods is aided by their being more or less insulated from the earth, but in any event the very existence of such a mass of metal as an old lightning-rod can only tend to produce a disastrous dissipation of electrical energy upon its surface,—"to draw the lightning," as it is so commonly put.

Is there a Better Means of Protection?

Having cleared our minds, therefore, of any idea of conducting electricity, and keeping clearly in view the fact that in providing protection against lightning we must furnish some means by which the electrical energy may be harmlessly dissipated, the question arises, "Can an improved form be given to the rod, so that it shall aid in this dissipation?"

As the electrical energy involved manifests itself on the surface of conductors, the improved rod should be metallic; but, instead of making a large rod, suppose that we make it comparatively small in size, so that the total amount of metal running from the top of the house to some point a little below the foundations shall not exceed one pound. Suppose, again, that we introduce numerous insulating joints in this rod. We shall then have a rod that experience shows will be readily destroyed—will be readily dissipated—when a discharge takes place; and it will be evident, that, so far as the electrical energy is consumed in doing this, there will be the less to do other damage.

The only point that remains to be proved as to the utility of such a rod is to show that the dissipation of such a conductor does not tend to injure other bodies in its immediate vicinity. On this point I can only say that I have found no case where such a conductor (for instance, a bell wire) has been dissipated, even if resting against a plastered wall, where there has been any material damage done to surrounding objects.

Of course, it is readily understood that such an explosion cannot take place in a confined space without the rupture of the walls (the wire cannot be boarded over); but in every case that I have found recorded this dissipation takes place just as gunpowder burns when spread on a board. The objects against which the conductor rests may be stained, but they are not shattered.

I would therefore make clear this distinction between the action of electrical energy when dissipated on the surface of a large conductor and when dissipated on the surface of a comparatively small or easily dissipated conductor. When dissipated on the surface of a large conductor,—a conductor so strong as to resist the explosive effect,—damage results to objects around. When dissipated on the surface of a small conductor, the conductor goes, but the other objects around are saved

A Typical Case of the Action of a Small Conductor.

Franklin, in a letter to Collinson read before the London Royal Society, Dec. 18, 1755, describing the partial destruction by lightning of a church-tower at Newbury, Mass., wrote, "Near the bell was fixed an iron hammer to strike the hours; and from the tail of the hammer a wire went down through a small gimlet-hole in the floor that the bell stood upon, and through a second floor in like manner; then horizontally under and near the plastered ceiling of that second floor, till it came near a plastered wall; then down by the side of that wall to a clock, which stood about twenty feet below the bell. The wire was not bigger than a common knitting needle. The spire was split all to pieces by the lightning, and the parts flung in all directions over the square in which the church stood, so that nothing remained above the bell. The lightning passed between the hammer and the clock in the above-mentioned wire, without hurting either of the floors, or having any effect upon them (except making the gimlet-holes, through which the wire passed, a little bigger), and without hurting the plastered wall, or any part of the building, so far as the aforesaid wire and the pendulum-wire of the clock extended; which latter wire was about the thickness of a goose-quill. From the end of the pendulum, down quite to the ground, the building was exceedingly rent and damaged. . . . No part of the aforementioned long, small wire, between the clock and the hammer, could be found, except about two inches that hung to the tail of the hammer, and about as much that was fastened to the clock; the rest being exploded, and its particles dissipated in smoke and air, as gunpowder is by common fire, and had only left a black smutty track on the plastering, three or four inches broad, darkest in the middle, and fainter towards the edges, all along the ceiling, under which it passed, and down the wall."

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SCIENCE

NEW YORK, MAY 19, 1893.

MR. HOLMES'S CRITICISM UPON THE EVIDENCE OF GLACIAL MAN.

BY G. FREDERICK WRIGHT, OBERLIN, OHIO.

MR. HOLMES has now concluded his series of reviews of the evidence of glacial man in America, having treated of the evidence from Trenton, N.J., and of that from Madisonville and Newcomerstown, Ohio, in the first two numbers of *The Journal of Geology*, published at Chicago, and of the Little Falls evidence, in Minnesota, in the April number of *The American Geologist*. It is, therefore, an appropriate time to make some remarks upon his criticisms. This I will do with as much freedom from prejudice as possible, and I think I am in position to be as free from bias as one can well be; for all along I have been in a strait betwixt two, being under pressure from my theological predilections to discredit the evidence, and accepting it at first with much misgiving.

A calm review of the case in the light of Mr. Holmes's criticism seems to make it probable that we have been mistaken about the character of Miss Babbitt's discoveries at Little Falls. Mr. Holmes seems fairly to establish the probability that the discoveries there made were either in the surface deposits or in a talus of the bank which had fallen down from the surface. But I will leave this for further discussion by those who are more familiar with the ground.

In case of the discoveries at Trenton, N.J., however, his criticisms fall far short of discrediting the abundant evidence that had been presented by other investigators, and this I say with what I believe to be pretty full knowledge of the facts and conditions connected with the discoveries — knowledge which I have derived from numerous personal investigations upon the spot and from frequent conferences with persons who have from time to time reported discoveries. But, as the discussion of this evidence in detail will more properly fall to some others who have more immediate cognizance of the facts, I will do nothing more here than simply to express the convictions of my mind after repeatedly reviewing the evidence on the spot since his criticisms.

The last paper of Mr. Holmes, however, treats of the reported discoveries in Ohio, whose discussion more properly falls upon me. The two discoveries upon which most reliance has been made in Ohio are that by Dr. Metz, at Madisonville, in the glacial terrace of the Little Miami River, and that of Mr. Mills, at Newcomerstown, in the glacial terrace of the Tuscarawas. Mr. Holmes urges two objections to the glacial age of the implement discovered by Dr. Metz at Madisonville, and with him I understand Mr. Leverett to agree. The implement was found some distance back from the margin of the terrace, where the material was finer than that facing the river, and occurred eight feet below the surface of the loam, in the upper part of the gravel. Mr. Leverett suggests that this loam may have been deposited later than the main part of the terrace. I do not, however, understand him to have any direct evidence of this, but simply to suggest it as a possibility. I am confident, however, that it is nothing more than a bare possibility, and that any separation of that portion of the terrace from that nearer the river is in the highest degree improbable. The glacial terrace is continuous from the river to Dr. Metz's house, and, according to the laws of the formation of such terraces, the finer material would be deposited back from the main stream in exactly the manner in which it is deposited there. We may therefore reject that supposition with a very great degree of confidence.

Second, Mr. Holmes and Mr. Leverett suggest that this implement may have worked down eight feet through the loam and into the gravel by the agency of upturned trees, or of the rotting tap-roots of oak trees. Professor Chamberlin has suggested to Mr. Leverett that probably fifty generations of trees had grown upon this spot. But it is difficult to see how the number of the generations of trees growing upon the spot would materially affect the question. The most that Mr. Holmes claimed in reference to the Little Falls locality was that implements might have worked down by the upturning of trees three or four feet into the surface soil. But fifty disturbances of the soil to a depth of three or four feet would not have the effect of one disturbance of eight feet. To go half-way fifty times does not produce the effect of going the whole of the way once. The supposition of the implement's having worked down through a tap-root as it decayed seems to rest upon so slight a probability that it is scarcely worthy of consideration. The necessity of resorting to such hypotheses to explain away each item of proof in detail will impress most reasonable minds with the extreme difficulty of resisting the evidence presented in favor of glacial man in America.

With reference to the Newcomerstown implement, there can really be no better answer to Mr. Holmes's criticisms than to reproduce, with a few critical remarks, two paragraphs in which he unconsciously reveals the attitude of mind with which he has approached the question. The paragraphs are taken from his article in the second number of *The Journal of Geology*, pp. 158–159, in the midst of which there are injected two beautiful fancy sketches, illustrating how he supposed the banks might have appeared when the implement was discovered. Here are the paragraphs:—

"Professor Wright is entirely satisfied with the results of his efforts to corroborate the statements of the collector. He has examined and re-examined Mr. Mills, receiving every assurance of the verity of the find, but, after all, he really secures no additional assurance and can receive no fully satisfactory assurance that Mr. Mills was not in error. Professor Wright has visited and photographed the site, and will speedily prepare a plate for publication, for just what purpose, however, it is rather hard to see, since the nature of the gravels is not disputed, and a volume of photographs will not give additional weight to the proofs. A photograph made of the tree after the bird has flown will not help in determining the bird. No more will observations on Mr. Mills's moral character, his education, or business reputation diminish the danger of error. The specimen may not have been found in place, notwithstanding all possible verification, and it may be a reject, notwithstanding its resemblance to foreign types, and Professor Wright may be wrong in urging his conclusions upon the public, notwithstanding his painstaking efforts to secure all possible affirmative testimony.

"It is nowhere stated that Mr. Mills actually picked the specimen out of the gravels; it was probably loose when he discovered it, but, even if he could say that it was fixed in the gravel mass, the necessity of questioning the find would still exist. All the authentication Professor Wright can possibly secure will not enable him to determine whether Mr. Mills struck with his walking-stick a small mass of the gravel in place at a depth of sixteen feet, or whether he was dealing with a mass which had slid with its inclusions of modern relics from the surface to a depth of sixteen feet."

In a former communication to *Science* (Feb. 3, 1893), I had promised to publish a more detailed account of this discovery, accompanied with a photograph of the bank. It is to this that Mr. Holmes refers. The promised publication appeared in the *Popular Science Monthly* for May, simultaneously with the article by Mr. Holmes in *The Journal of Geology*. Doubtless it will strike the reading public rather strangely to have Mr. Holmes

speak so slightingly of the value of a photograph of the bank showing it as it actually was soon after the discovery, when he has himself given two fancy sketches, representing an impossible condition of things, to inform us how he thinks it might have been. The photograph of the bank taken by Mr. Mills, within six months of the time of his discovery, exhibits its face intact, and is a part of the evidence presented as to what was the actual condition of the gravel when the discovery was made. The haste with which Mr. Holmes has plunged into this discussion is shown by his statement on a previous page that Mr. Mills had "published nothing save through Professor Wright." The report of the Western Reserve Historical Society referred to by Mr. Holmes is entitled a report "by Mr. Mills and Professor Wright," and the specific account of the discovery is given in Mr. Mills's own words, in which he says that when a space of about six feet in length by two in height fell down, it exposed the specimen to view. It is true that that statement is not so explicit as it should have been, and I have given, in the *Popular Science Monthly*, the fuller details as given to us upon the spot, and as repeated two or three times to me in correspondence, namely, that the implement was seen by him projecting from the face of the gravel bank after the fall of gravel before referred to, and when the edges of the strata of gravel were all visible and undisturbed, and that he took it out with his own hands; or, if you want to avoid all error, that he worked it loose with his walking-cane until it fell out at his feet, when he took it up, made his notes upon it, and put it in his collection. Mr. Mills is as capable of drawing a section of the bank as Mr. Holmes is, and that he has done, but most readers will prefer to see a photograph, in which there is no danger of the incorporation of fanciful elements.

In view of all that Mr. Holmes has said of the importance of expert testimony, it is difficult to see, also, why he should say that observations upon Mr. Mills's moral character, education, and business reputation may not diminish the danger of error in such a case; for how else can you determine the value of an expert's testimony? If there is doubt about his moral character, that of course vitiates the evidence in a high degree. So, also, if there is doubt about his ability to discern the difference between disturbed and undisturbed gravel in such a situation, that would largely vitiate the observations. But Mr. Mills's education and habits of observation are such that his evidence in so clear a case as this is, is as good as that of any expert could be. What does Mr. Holmes suppose led Judge Baldwin and the other members of the Western Reserve Historical Society to incur the trouble and expense of going down to Newcomerstown, except it was to inform themselves of the capacity of Mr. Mills to bear testimony to the very points at issue? Of course, we cannot force conviction upon the minds of the public, but we can get the facts of the situation and the conditions under which the evidence was given with all possible clearness before them. If any portion of the reading public chances to be in the attitude of mind in which Mr. Holmes asserts he is in when he says he does not care for a photograph of the bank, and does not care to know anything about the moral character and education of the witness, and that he is sure that Professor Wright cannot possibly secure a proper authentication of the facts, it will be a difficult matter to overcome the prejudice with which the subject is approached. But the number who are biassed to such an extent and are the subjects of such "invincible ignorance" is, I presume, not numerous.

Of course, I do not deny that there are things so improbable that they could not be established by any amount of human testimony. It is more likely that the senses should be deceived in some cases than that the things which seem to happen should really occur. But this is not a case of that sort. The existence of glacial man is not a highly improbable thing, and this evidence of Mr. Mills is in analogy with a vast amount of other evidence leading to a similar conclusion. There is nothing in the character of the implement, in the conditions under which it is reported to have been found, or in the testimony presented, to raise any serious suspicion of error. The fact that Mr. Mills was not specially impressed by the importance of the discovery at the time is not at all surprising, since his thought had been little directed to the phase of the subject involved in his discovery. He had in his collection thousands of other implements found upon the surface, and, after making note of the circumstances connected with the finding of this, it was laid with them.

In conclusion, I would simply add that in procuring, as I have done during the past season, some sections of the gravel in undisturbed condition for the exhibit at Chicago, I have had ample opportunity to study its behavior, both when it is in place and when it is in a recently formed talus, and, in reply to Mr. Holmes's assertion that it is *impossible* to tell whether Mr. Mills found this in the undisturbed strata or in the talus, I would say that the observer who could not tell the difference would be one whose testimony was utterly unworthy of consideration. While I am about it, also, I might as well refer to the fact that there is a slight discrepancy, which may attract the attention of some, both in my own and in Mr. Mills's statements about the depth at which the implement was found. In "Man and the Glacial Period" I say, that it was *sixteen* feet. In my original report upon it, I say *fifteen* feet. In the more specific details given in the *Popular Science Monthly* I say *fourteen and three-fourths* feet, and Mr. Mills has sometimes spoken of it as fifteen feet and sometimes as fourteen and three-fourths feet. It is easy enough to see why both of us should say fifteen feet, for that is a round number, but not so easy to see why in one place I should have said sixteen feet. But the discrepancy is not one that materially affects the evidence. I presume, therefore, that my error arose from the principle of assimilation with which we are so familiar in the textual criticism of the New Testament. In the appendix to the third edition of my "Ice Age in North America," I give it as fifteen feet. But in writing the paragraphs in the later book, I had just had occasion to speak of one of Dr. Abbott's discoveries which was sixteen feet below the surface, and the close association of the two in my mind doubtless led to the substitution, and, since there was nothing specially dependent upon it, the discrepancy being so slight, my attention was not aroused through all the subsequent proof-readings.

PHARMACEUTICAL EDUCATION.

BY HENRY KRAEMER, COLLEGE OF PHARMACY OF THE CITY OF NEW YORK.

DURING the past year a number of papers have appeared in *Science* demonstrating the "onward march" of institutions of the highest learning, as well as that of professional and technical schools in America. The one cry to be heard all along the line is to raise the standard. The requirements for a preliminary education have been markedly increased and the courses of studies materially lengthened both as to the number of hours required per week and the years of study. In our colleges of pharmacy there have been a similar awakening and a desire to extend the course from two to three years. It may be well, however, at this point to state for the benefit of those who are unfamiliar with the requirements of our best colleges of pharmacy, that before a diploma is granted the student must have been engaged in the drug business for a period not less than three and one-half or four years. This means practically an apprenticeship of six years, although a great many students find it necessary to work in drugstores while attending colleges.

The teachers of pharmacy have for a number of years been discussing ways by which students will be compelled to devote all of their time to college work during the sessions of study. Yet while they claim that students should not be employed as clerks in the stores and at the same time attend college, the employers are opposed to the students devoting so much of their time to college work during the winter session. There has been more or less of a compromise, but nevertheless colleges of pharmacy are raising their standard as are the other schools of learning, and it is very probable that, in a few years, three solid sessions of undivided work as well as fours years' apprenticeship will be required before a candidate shall receive his or her degree.

The position of the pharmacist is a peculiar one. He, in the majority of cases, does not make his living by means of his actual business in medicines and prescriptions. He finds it necessary to

carry a line of goods known as "druggists' sundries" and "patent medicines." These, and more especially the latter, he would give up if he could, but the line of competition is so great and the public still expect the pharmacist to carry any-and-everything to suit their convenience, that it seems only practicable to a very few to abandon these in their business. The public also expect the pharmacist to know something of everything, and whatever it be, whether ills or troubles or discomforts of any kind, they run to him. I remember, when attending college, one of the professors, who was a practising pharmacist for a number of years in one of the best localities in a large city, telling us that one night he was hastily summoned by a neighbor to his house, where, in the midst of a splendid reception, the gas had suddenly gone out, and, not knowing what to do, they sent for the pharmicist. He went, and being of a practical mind and true to the instincts of his discomforted neighbor, he remedied the trouble. This simply illustrates the very close relations of the pharmacist to the public.

Now, as soon as the public will expect the pharmacist to deal in medicines only and all other articles related to the art of medicine, then the pharmacist as a business man (which he must be) will confine himself to the labors of his profession. And as soon as he can confine himself solely to the art of pharmacy as taught in our colleges, there will be no question of an extended curriculum of studies, as complete as that of any institution of learning. Then we shall have laboratories fully equipped in the particular kind of analytical and chemical apparatus which he needs for the assay of drugs and in their examination for purity. Likewise will the course in microscopical work be so extended that the pharmacist will make such analyses, for the busy physician, as the examination of urinary sediments and other discharges, such as sputum for tubercle-bacilli, etc. Indeed, it is in these two fields that the advanced work in pharmacy is tending, and accurate results will only be attained by thorough instruction in chemical and microscopical manipulation. There must be such a blending of chemical and botanical instruction that the pharmacist, while not a specialist as a chemist or a botanist, yet indeed is a specialist with regards to the practical application of these sciences as an aid to the physician in his healing art and in the preparation of pure medicines of definite and authorized strength. This condition of specialization will come, for pharmacists are marching onward in the line of progress; and it is only a question of a few years, when the host of young men, graduating by the hundreds from our colleges of pharmacy, and who are thirsting to apply their teachings and make their living in this practical application, will unite and raise the standard of their business to the profession which it is theirs to make it.

THE TELL EL-AMARNA TABLETS.

BY THE REV. THOMAS HARRISON, F.R.G.S., MEMBER OF THE SOCIETY OF BIBLICAL ARCHÆOLOGY, AND SENIOR LECTURER TO THE PALESTINE EXPLORATION FUND, STAPLEHURST, KENT, ENGLAND.

THE Tell el-Amarna tablets, after some years of patient study on the part of experts, are now known to consist for the most part of a political correspondence of great interest and importance between kings, governors, and officers, who formed their plans, struggled with their difficulties, fought their battles, and made their exit from the worry and work of life 3,370 years ago. These letters are inscribed on brick tablets, and, as a rule, occupy both sides of the tablet. With two exceptions, which are from Hittite princes and in their language, the letters are written in an ancient form of the cuneiform script. They were found in the year 1887 by an Egyptian peasant woman amid the ruins of the palace of Amenophis IV., or Khu-en-Aten, at a place now known as Tell el-Amarna, midway between Minieh and Assiout, on the eastern bank of the Nile, about 180 miles by river south of Cairo. The tablets number 320. The writers of the letters from Palestine (178 in number) are Amorites, Phœnicians, Philistines, and others, and they are addressed to the Pharaoh of Egypt and certain of his officials. At the time of this correspondence (about 1480 B.C.) the power of Egypt was waning and Egyptian garrisons were being withdrawn from Palestine in face of successful attacks by the kings of Armenia, Nii, Shinar, with the Hittites of Merash and Kadesh on the north, and of equally successful attacks by the *Abiri* (Hebrews) on the south. The letters state that the Abiri came from the desert and Mount Seir. Major Conder affirms that "the date of the letters is exactly that which is to be derived from the Bible (I. Kings vi., 1) for the Hebrew invasion, according to the Hebrew and Vulgate text, and it agrees with the fact that the Egyptian conquests made by the XVIII. dynasty (1700 to 1600 B.C.) had been lost when the XIX. dynasty acceded." It is certainly very interesting to find in the letters the names of Japhia (Josh. x., 3, one of the kings killed by Joshua) and most probably that of Adonizedek, king of Jerusalem; while the name of a king of Hazor is read as Jabin (Josh. xi., 1). It is also pointed out that the name of the captain of Jabin's host is, Egyptian, Sisera or Ses-Ra, meaning servant of Ra.

In most of the letters from the kings of the cities of Phœnicia and Northern and Southern Palestine the appeal is ever one for Egyptian troops to enable them to hold their cities for the Pharaoh, to whom they seem to have appealed in vain. The earlier letters of brave Ribadda, the king of Gebal (now Jubeil, north of Beyrout), usually begin with the following salutation, which is given as a specimen of such salutations at that time, "Ribadda of the city of Gebal of his Lord, the King of many lands, the prosperous king, Baalath of Gebal, she hath given power to the King my Lord. At the feet of the King my Lord, my Sun seven times seven times I bow."

The salutation of the later letters becomes shorter and less ceremonious, as Ribadda felt that he was being left to his fate. Here is one of his appeals for help: "I have been hard pushed. Help speedily O King my Lord. . . . Soldiers and chariots, and you will strengthen the chief city of the King my Lord."

And what can be more pathetic than this, coming from that same brave heart, which has now for more than 3,300 years ceased to trouble itself about chariots and men of war and Pharaohs who could not or would not come to his aid.

"And will not my Lord hear the message of his servant? Men of the city of Gebal, and my child, and a wife whom I loved, this son of war, the son of Abdasherah has seized; and we have made a gathering, we have searched; and I cannot hear a word spoken about them. I am doing my duty to the King my Lord, and once more, despatch thou men of garrison, men of war, for thy servant, and will you not defend the city of the King my Lord?"

On May 14, 1892, a cuneiform tablet was found by Mr. Bliss while excavating at the old Amorite city of Lachish, in Judea, in which the name Zimridi twice occurs. From the Tell el-Amarna tablets we learn that Zimridi was governor of Lachish, and, moreover, in a tablet from the king of Jerusalem to Amenophis IV., we are informed of the death of Zimridi at the hands of the servants of the Pharaoh just named.

Many matters of great interest in connection with these tablets can find no mention within the limits of this paper. It may be added, however, that the topographical value of these letters is very great; and also that the evidence which they afford as to the Hebrew conquest of Palestine under Joshua is in favor of the Bible chronology (Acts xiii., 20; I. Kings vi., 1) and against that of Dr. Brugsch and Bunsen.

SOME CONFLICTING ESTIMATES OF DISTANCE.

BY ARTHUR E. BOSTWICK, PH.D., MONTCLAIR, N. J.

ACCORDING to all authorities with which I am familiar, a small, regular pattern, if looked at squintingly, so that the horopter is nearer the eye than the pattern, but at such a distance that adjacent corresponding parts of the latter overlap and coalesce, should appear closer to the observer, and if looked at in like manner, but so that the horopter is farther from the eye than the pattern, it should appear farther away. This seems natural, for, in each case, the image on the retina being unblurred, the point to which the axes of the eyes converge should be taken as the distance of the object. In this case, the angle actually subtended by the pattern remaining the same, the mind should infer, in the

first case, that the pattern has grown smaller, and, in the second case, that it has grown larger.

The writer of this note has never been able to make things appear to him in this way. When the horopter is nearer than the object, the pattern, though it appears smaller, seems also distinctly more distant, and when the horopter is farther away, the pattern seems larger and nearer. When one has learned the trick of causing the adjacent parts of the pattern to overlap and coalesce perfectly, the experiment may be tried as often as one likes, and I have tried it often and under many different conditions, always with the same result. Of course, care has always been taken to make sure of the point at which the axes of the eyes converge, either by converging them at first on the tip of the finger and then removing it, or by moving the finger to and fro in the field of vision after the eyes have become fixed, the separate images becoming closer together or farther apart, according as the finger approaches or recedes from the horopter. The fact is, as is well known, that an estimate of an object's distance is always an inference from various data furnished by the eye, as the visual angle, the position of the horopter, and the muscular movement in each separate eye necessary to effect accommodation. For distant objects the last mentioned fails, and aërial perspective comes in to aid; but for objects that can be used in this experiment the three factors mentioned are those on which the eye relies. The conditions in the experiment being unique, the data obtained are discordant, and it is not wonderful that different persons, under the circumstances, disagree in their estimates of the distance of the pattern.

Take the case where the eyes are squinted. The pattern being seen clearly, and no accommodation being necessary, each eye separately infers that the object has remained stationary. The horopter having advanced, the two eyes jointly agree that the pattern is nearer. But, if it is nearer, the angle it subtends remaining the same, the pattern must be actually smaller. But, on the contrary, no accommodation for bringing it nearer has been necessary, so, if it is smaller, that must be an apparent effect due to its having moved back. The conclusion to which one comes must be influenced by the relative weight that he is unconsciously accustomed to give to the different data on which his estimate of a distance is ordinarily based. And having interpreted the phenomena in one particular way at first sight, this becomes habit, and what may have been determined by chance the first time one tries the experiment becomes a settled thing. Often as I have tried it, however, I am always conscious of a queer feeling of surprise as the pattern comes out clearly before me — a feeling that all is not quite right, due, of course, to the unconscious clashing of these contradictory data. I may add that in my own case, and I suppose in that of others, in monocular vision an object appears distant or near as the eye is fixed respectively on something nearer than it or something beyond it. As accommodation is associated always with concentration of the axes of the eyes, it is doubtless impossible to accommodate the focus perfectly to the pattern while the horopter is in a different plane, hence, as in the case of the writer, this may tip the balance in favor of his peculiar way of inferring from the clashing data.

NOTES AND NEWS.

ALL lovers of ferns will be glad to learn that an association for the study of these plants by correspondence has been formed. The work will be made as easy as possible for beginners, and all who are interested in ferns are invited to join. Applications should be made to the secretary, Miss A. May Walter, 516 Spruce Street, Scranton, Penn., or to Willard N. Clute, Binghamton, N.Y.

— Professor Daniel G. Brinton, M.D., LL.D., of Philadelphia, received on May 10 the further honorary degree of "Doctor of Science" from the University of Pennsylvania. His works are numerous, and have been principally upon linguistics, ethnology, and American archæology.

— The Chicago Academy of Sciences has undertaken the collection of views from all localities in Illinois, and adjacent parts of Indiana, Michigan, and Wisconsin, for the purpose of bringing together, where they may be accessible to all scientific workers, a complete series illustrative of the geological and natural history features of the region. The value of such a collection is apparent, and the Academy believes that, in the interest of science, it may reasonably expect the coöperation of all who may be in a position to assist in the work. While all views are acceptable, those illustrating the following features are especially desired: geology, topography, land, water, and forest scenes, farm life, public buildings, neighborhood characteristics, and, in general, anything characteristic or unique in the study of nature or man. In sending views, please observe the following directions: 1. Send photographs unmounted. 2. Send with each a careful description of (*a*) the locality, (*b*) objects shown, (*c*) direction of view, (*d*) by whom taken. Number descriptions and views to correspond.

— A meeting of the Victoria Institute was held at Adelphi Terrace on May 1, at which an address by Professor Maspero, embodying the results of his investigations during the past ten years as regards the places in Southern Palestine claimed, according to the Karnac records, to have been captured by the Egyptians in the campaign under Sheshonq (Shishak) against Rehoboam. M. Maspero pointed out the great help that the recent survey of Palestine had been in determining the localities referred to, and specially referred to the fact that the Egyptian letters, rigorously transcribed in Hebrew letters, gave almost everywhere the regular Hebrew forms in the Bible, "without change or correction." The paper was admirably read in the author's absence by Mr. Theo. G. Pinches of the British Museum, who afterwards added some remarks. The discussion was continued by several members, including Major Conder, R.E., who contributed many interesting details. During the discussion reference was made to the great interest taken in the question by the late Canon Liddon, who, on the occasion of Professor Maspero's former paper being read, pointed out that the identity of form of the words in the Egyptian and Biblical records pointed to the antiquity of the latter.

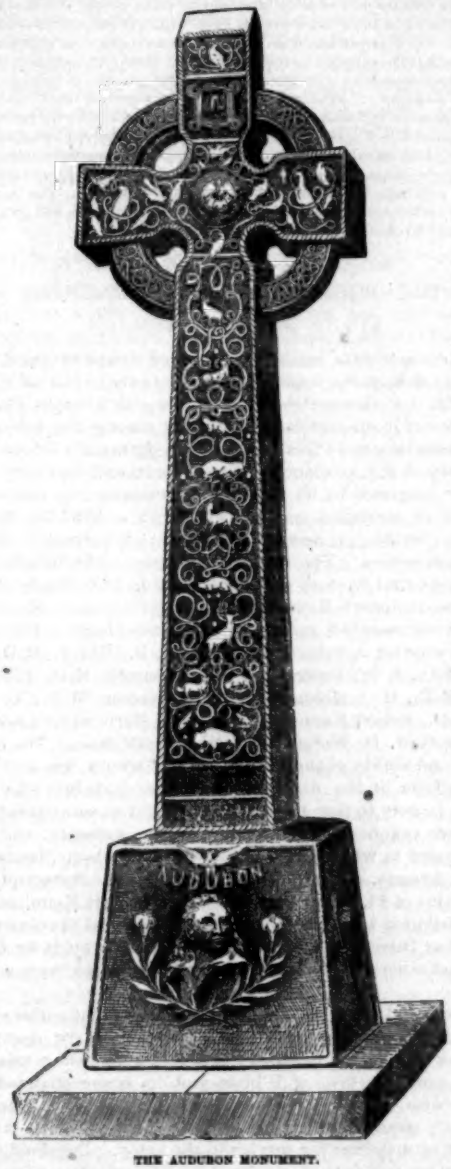
— Morris Phillips & Co. have issued a new edition for 1893 of "Abroad and at Home." This book is a guide of an unusual character, giving much information in regard to hotels, boarding-houses, restaurants, etc., of considerable value to those who intend to go abroad, or who intend to travel in this country. Last year, the book first appeared early in the summer, and during the three summer months three editions were called for. This year, new matter has been inserted descriptive of Atlantic City, Niagara Falls, the St. Lawrence, Adirondacks, and Saratoga Springs, and a summer-resort guide giving information regarding the leading hotels. A specially prepared chapter on Chicago also appears.

— "Miss Helen Keller, who may be regarded as the most remarkable person in this country when her natural deficiencies [blind and deaf] are compared with her graces and gifts," says *The Evening Star* of Washington, of May 11, "is now a guest at the house of Mr. Alexander Graham Bell in this city. Last evening a number of well-known gentlemen were also his guests and had an opportunity to see how extraordinary is the intelligence of this young lady and how more marvelous is her power of expression, not only by manual signs, but also by distinct and agreeable oral utterances. Among the guests was Senator Sherman. Professor Bell said to Helen: 'This is the birthday of Senator Sherman and we are going to drink his health. We want you to propose a toast. Do you know what that means?' As this was a new idea to the young lady it was explained to her. 'We want you to propose a sentiment in honor of this birthday,' said Professor Bell. Helen looked puzzled or thoughtful for a minute and then said slowly and with a sympathetic emphasis: 'I propose his health, happiness and prosperity. May he be as helpful to his country in the future as he has been in the past, and may he be blessed with all good things in this life and in the beautiful life to come.' During the evening the quickness and fitness of her answers to Professor Newcomb and other scientific gentlemen surprised everybody. So did her accurate repetition of Longfellow's Psalm of Life, and so did her keen enjoyment of stories told to her and of the conundrums with which she puzzled the friends who were talking with her. Her story is wonderful, and the skill of her teacher, Miss Sullivan, is admirable in the

highest degree. Besides Senator Sherman the following gentlemen were present: Senator Morrill, Professor Langley, Professor Gilbert, Maj. Powell, Professor D. C. Bell, President Gilman of Baltimore, the Danish minister, Mr. G. G. Hubbard, Mr. Pollok, President Gallaudet, Professor Newcomb, and Col. Britton. Here is what Miss Keller got off on one of the justices of the Supreme Court, who called to pay his respects on the occasion of the reception given her by Mrs Graham Bell the other evening: Helen asked the justice: 'Do you know my friend, Judge Holmes?' 'No, dear, he lives in Boston.' To which she replied, smilingly: 'Oh, I thought you knew him, because you see you are brothers-in-law.' The justice took in the bon mot and laughed heartily."

— The institution which was founded and endowed a few years ago in Washington by Professor Alexander Graham Bell for the increase and diffusion of knowledge relating to the deaf, and which has heretofore had its headquarters at 1334 Q Street, is to have a new home. The institution is styled the Volta Bureau, and in the past two or three years has issued a number of valuable books upon the education of the deaf. The work of the bureau has increased to such an extent that it has been found necessary to provide new quarters. To this end, ground was broken, within a few days, on the northeast corner of 35th and Q Streets for the proposed building, which has been planned and upon which work will be commenced at once. The first sod was turned by Miss Helen Keller, the remarkable deaf and blind girl whose history and wonderful development is known to readers of *Science* through recent publications. The ceremony of breaking ground was also participated in by Miss Elsie May Bell, Miss Marian Hubbard Bell and Master Douglass McCurdy.

— Rarely has a more interesting and beautiful memorial been raised to the memory of a man devoted to science than the monument lately unveiled in New York City to the eminent ornithologist, J. J. Audubon. The ceremonies took place on the afternoon and evening of April 26, and consisted of the dedication of the monument in Trinity Cemetery at 3.30, and a public meeting and addresses at 8.30 in the hall of the American Museum of Natural History, the principle address being a noble eulogy on Audubon, by Professor D. G. Elliott, president of the American Ornithologists' Union. The movement has been carried out chiefly through the agency of the New York Academy of Sciences. It was begun six years ago, and originated with Professor Thomas Egleston of the Columbia College School of Mines, who noticed the fact that Audubon's remains had lain for over thirty years in an obscure vault in a remote corner of Trinity Cemetery, almost unmarked, and wholly undistinguished by any proper memorial. He enlisted the interest of a few scientific friends, and the coöperation of the trustees of the cemetery, who offered an excellent site, free of expense. The matter was brought before the American Association for the Advancement of Science during its meeting in New York in that summer (1887) by Professor Daniel S. Martin, but no formal action was taken. At the opening of the meetings of the New York Academy of Sciences, in the autumn, Professor Martin again presented the subject, and a committee was appointed, consisting of Professor Egleston, as chairman, and Drs. N. L. Britton and D. S. Martin. To the labors of this committee, and especially of its chairman and secretary, Professors Egleston and Britton, the result now attained is due. Other societies were invited to coöperate, and have done so to some extent, especially the American Ornithologists' Union and the Agassiz Association. The amount sought was $10,000. About five hundred persons have subscribed in varying amounts, the whole exceeding the sum proposed, while the monument has cost somewhat less. A balance of over $1,000 will remain, which is to be held in trust permanently, as an "Audubon Memorial Fund," by the Academy of Sciences, and used for the publication of important scientific memoirs on subjects kindred to those of Audubon's studies and pursuits. The monument itself is a noble and striking work. It stands on a beautiful knoll, close to the 152d Street entrance, facing the point where Audubon Avenue is to be opened through to that street from the north, and close to the old estate, Aububon Park, where the great ornithologist passed his later years. It consists of a Runic cross, some fifteen feet high from the base, and is richly carved with appropriate designs, this treatment being possible, historically and aesthetically, upon the Runic cross. The nearly cubical base bears on its front a medallion head of Audubon; on the back an inscription of the manner of its erection, through the New York Academy of Sciences; and on the two sides designs of the hunter's and artist's outfit, respectively, with flowers particularly noted or described by Audubon. The shaft and arms of the cross are elegantly carved in scroll-work, interwoven with a series of birds



THE AUDUBON MONUMENT.

and animals of characteristic North American species, on the front and back respectively. The whole is unique and impressive, carefully studied in both its scientific and artistic details, and singularly happy and appropriate in conception. The material (Hudson River blue-stone) lends itself admirably to the work thus wrought, and the whole rests on a substantial granite base. The spot is beautifully laid out and kept by the trustees of the cemetery, and the whole enterprise reflects great credit on those who have planned and executed it, and is an honor to the Academy of Sciences, an ornament to the city, and a fitting tribute to the memory of the great ornithologist.

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THE CHICAGO ACADEMY OF SCIENCES.

BY C. H. GORDON, EVANSTON, ILL.

THE arrangements recently completed whereby the Chicago Academy of Sciences receives from Mathew Laflin of Chicago $75,000 for the construction of a building at Lincoln Park, revives interest in an association formerly among the foremost of similar associations in this country. In giving a brief résumé of the history of the Academy, the treatment will naturally follow the order suggested by its financial experiences, as follows: (1) period of organization and early struggle — 1856–64; (2) prosperity — 1864–81; (3) decline — 1881–91; (4) revival — 1891.

1. *Organization.* The Chicago Academy of Natural Sciences, as it was at first termed, was established in 1856, chiefly through the efforts of Robert Kennicott, then a young man of indomitable perseverance and rare scientific attainments. There were fourteen charter members as follows: J. B. Blaney, M.D., N. S. Davis, M.D., J. W. Freer, M.D., C. Helmuth, M.D., E. W. Andrews, M.D., H. A. Johnson, M.D., H. Parker, M.D., F. Scammon, M.D., Robert Kennicott, George A. Mariner, Samuel Stone, R. K. Swift, J. D. Webster, H. W. Zimmerman. The energy, loyalty, and ability of these men, some of whom are still active in the affairs of the Academy, gave the impetus which contributed largely to the success attending it in subsequent years. Steps were taken very early to establish a museum, and rooms were secured in what was then known as Dearborn Seminary on Wabash Avenue, north of Washington Street. Subscriptions to the amount of $1,500 were obtained, and Robert Kennicott, who had contributed his fine collection of zoölogical specimens, was installed as Director. Contributions were also made by Dr. Andrews and others until many thousand specimens were accumulated.

The financial crash of 1857 ruined most of the subscribers to the original fund, and left the Academy nearly destitute of means for meeting its necessary expenses. In 1859 it was incorporated under the laws of Illinois and its name changed to its present form. The Civil War breaking out soon afterwards completely absorbed public attention, and took from its ranks the more active members for service in the army. Deprived of public attention, destitute of money, and stripped of working members, the whole enterprise seemed likely to be extinguished.

In 1859, Robert Kennicott departed for Arctic America in charge of an exploring expedition under the auspices of the Smithsonian Institution. He returned from Alaska in 1863, richly laden with specimens in all departments of natural history. The results of this exploration were considered to be second to no other similar expedition on record. As the expenses of the expedition were borne by the Smithsonian Institution, the collections all went to Washington with the understanding, however, that a full series of the specimens was to be presented to any institution, otherwise eligible to such donation, that Mr. Kennicott might designate, provided suitable provisions were made for their reception and preservation. Naturally, Mr. Kennicott designated the Chicago Academy of Sciences as the recipient of such donation. During the year 1863 the collections were studied and arranged, and early in 1864 word was received stating that the duplicate series awaited the action of the Academy.

2. *Period of Prosperity.* The opportunity thus offered was eagerly seized by the active spirits remaining, and steps taken to secure the requisite funds. On Feb. 22, 1864, an informal meeting of interested gentlemen was held, at which Professor Agassiz, who was opportunely visiting Chicago, was present. Professor Agassiz testified to the great value of the collection and gave added impulse to the movement.

A reorganization of the Academy followed, and a corresponding act of incorporation obtained in 1865. A change in the constitution was effected by which life memberships of $500 each were established. During the summer of 1864, an active canvass was made resulting in 125 subscribers to life-memberships, making a total of $62,500. Of this, $50,000 was to be held as a permanent endowment. The following year the collections were received and deposited temporarily in the Metropolitan Building, at the corner of Randolph and LaSalle Streets.

The selection of a lot and the erection of a building next engaged the attention of the Academy, and it was only after much effort and discussion that a location was decided upon; but finally in January, 1867, the trustees reported the purchase of a lot on Wabash Avenue, just north of VanBuren Street, and the construction of a fireproof building thereon soon after began. Here began the contention, sometimes characterized by good humor, sometimes by acrimony, and renewed from time to time through the following years between the Academy and its trustees. The cause of this contention lay in the constitution, which was defective in two points; first, in delegating to the trustees, a body of its own creation, sole authority in certain lines, thus depriving itself of supervision over the acts of its servants; and, second, of failure to define clearly the powers and limits of the two bodies. A dual government was thus inaugurated, destined to bring trouble and perplexity into the affairs of the Academy. The assets of the Academy at this time, as shown by the financial report, were $72,000, with an annual income of $6,500. In the meantime, however, it had suffered two irreparable losses. The first was in the death of Robert Kennicott, which occurred at Nulato, a Russian port on the Yukon river, May 13, 1866, while on a second expedition to the far north, which had been undertaken the previous summer. Following close upon this (June 7th) came a disastrous fire in the building containing the collections, by which a large portion were destroyed and the remainder badly damaged.

Notwithstanding these misfortunes, however, the years from 1864 to 1871 may be reckoned as the most prosperous years in the history of the Academy. Active investigations of much importance were being carried on in various lines, and the work of its active members attracted the attention of leading scientists throughout the country, many of whom were enrolled as corresponding members. In meteorology, observations were conducted under the special supervision of Dr. Joseph Henry of the Smithsonian Institution. The work in natural history, which had received special impetus from the labors of Robert Kennicott, was continued under the able leadership of Dr. Stimpson, and the growth of the museum was remarkable. The library was in constant receipt of books and the publications of scientific associations of all countries, while the papers presented to the Academy were of a high order of merit.

The Academy building was completed in 1867, and the association immediately entered upon the publication of its proceedings, the first part of volume one being issued in 1868 and the second in 1870.

On the departure of Mr. Kennicott, in 1865, Dr. W. S. Stimpson, a young man of rare scholarship and excellent scientific attainments was called to act as director of the museum, to which duties were added those of the secretaryship. The Academy was especially fortunate in having at the helm in its early career two men of such eminent ability, energy, and devotion as Kennicott

and Stimpson. During these years Dr. Stimpson was actively engaged in investigations, the published results of which would have made his a well-known name among the scientists of the world.

In 1871 came the great fire destroying the Academy's building with all it contained, sweeping away all the results of Dr. Stimpson's life-work, as well as swallowing up in the general ruin the private fortunes of the most active supporters of the Academy. The loss of his papers was a severe blow to Dr. Stimpson, from which he never recovered. After the fire he was taken to Florida, where he died the following May.

At the beginning of 1872, the assets of the Academy, exclusive of the lot, were $23,000, $10,000 of which represented the insurance on the burned building. No money was available for building, but it was decided to borrow and erect on the lot two buildings, one for the museum and one for rental. The courage and hopefulness thus evinced was but a part of that characterizing Chicago after the fire, and, as in the case of many a private interest, the too sanguine view was but the prelude to further disaster. The buildings were completed in 1873, involving a financial burden of $80,000, afterward increased to $100,000.

In the general depression of business following the fire, the income of the Academy was insufficient to meet expenses and interest, until in 1881 the mortgage was foreclosed and the society was homeless.

During this time, however, the scientific work was carried forward with commendable zeal and success. The records show the interest to have been well sustained and the papers meritorious, while the museum prospered notwithstanding the financial stress.

3. *Decline.* Following the loss of the property, interest flagged, hope died out, and for ten years it became a bare struggle for existence. The museum building was retained by rental for two years, after which the collections were transferred to the Exposition Building, where they remained for several years under the care of the curator, J. W. Velie. The meetings were desultory and not well sustained. Two series of valuable bulletins were issued, however, during this period.

4. *Revival.*— In 1891 it was decided by the city authorities that the old Exposition Building should be removed. This revived the question of the disposition of the collections. A proposition involving its transfer to Chicago University was not favorably received by many of the members, when an opportune benefactor appeared in the person of Mathew Laflin, and settled its location at Lincoln Park. This agreement contains a provision by which the commissioners of the park are to add $25,000 toward the erection of the building and to bear all the running expenses, including salary of curator and assistants to an amount not exceeding $5,000 annually. The final arrangements were completed April 1, 1893, since which plans have been accepted and the construction will soon be under way.

Within these two years interest in the Academy has greatly revived, many new members have been enrolled, and active investigations set on foot along many different lines. Sections have been formed in astronomy, microscopy, chemistry, and other lines of work.

The disposal of the museum frees the Academy from a heavy burden, thus making the income available for publications which are to be renewed at once.

One of the enterprises now engaging the attention of the Academy is a geological and natural history survey of Chicago and vicinity. This will include the preparation of a topographic map of the area on a scale of about one and one-half inches to the mile, with contour intervals of five feet, and accompanying reports upon the geology, paleontology, zoölogy, botany, and archæology of the district. The work is in charge of a board of managers, and is being prosecuted as actively as possible. In the preparation of papers many noted scientists both in and out of Chicago are giving assistance.

In connection with this work the board has also undertaken the collection of views from all parts of Illinois and adjacent parts of Indiana, Michigan, and Wisconsin, illustrating interesting features of geology, topography, and other points of interest. These will be mounted, classified, and deposited in the Academy building, where they will be accessible to all who may wish to consult them.

The president of the Academy is Dr. S. H. Peabody, ex-president of Illinois University and superintendent of the Liberal Arts exhibit at the World's Fair. Dr. Peabody has been an active worker in the Academy for many years.

The present hopeful outlook for the society must be attributed in large measure to the untiring zeal and energy of its efficient secretary, Professor W. K. Higley. Among those identified with more or less of the history of the Academy the following are still among its loyal supporters: Dr. E. W. Andrews, G. C. Walker, E. W. Blatchford, B. W. Thomas, B. F. Culver, C. M. Higginson, Professor G. W. Hough, Dr. N. S. Davis, S. W. Burnham, S. H. Peabody, and others. Prominent in the past but no longer appearing on the active roll are the names of Professor M. Delafontaine, E. Colbert, J. D. Caton, Professor H. H. Babcock, ex-Gov. Wm. Bross, J. H. Rauch, J. W. Foster, and others.

IS IT A SCIENCE?

BY WILLIAM L. SCRUGGS, ATLANTA, GA.

IN the current discussions of international questions we often encounter the words commonwealth, state, and nation in the alternate form, as if they were synonymous and convertible terms. Now, a commonwealth may be a state or a nation, or both; a state or a nation may be a commonwealth. But the term nation implies the unity of a people of the same race, descent, and language under one government; whereas a state may be composed of people of diverse origin united under one government of whatever form; whilst a commonwealth is the unity of a people under a free or representative government.

Again, we have the commonly accepted statement that "states or nations are bodies politic or societies of men united together for the purpose of promoting their mutual safety and advantage by the joint efforts of their united strength." This is Vattel's definition, derived from Cicero. But states and nations are not equivalent terms, nor are "societies of men united together for the purpose of promoting their mutual safety and advantage" necessarily either "states or nations." The old Hudson Bay Company was such a "society of men united," but it was neither a nation, state, or commonwealth. Pirates and robbers are so united, but they have none of the essential elements of statehood. The political bodies corporate in the United States, the people of which constitute our national government, are literally within Vattel's definition; but they are neither "states" nor "nations" in the strict legal sense. They have a local police system or automatic government, but none of the elements of sovereignty or nationality. The very form of their local autonomy is prescribed by a superior power; they can have no diplomatic relations even between themselves, much less with foreign powers; they cannot declare war or enter into public treaties; they cannot establish post-offices and post-roads; they cannot levy and collect import duties; their very local legislation must conform to that of an external and paramount authority; and their citizens are such only by reason of the fact that they are citizens of the United States. Hence, so far from being "sovereign," these political bodies corporate are not even "states" in any just sense. They would be more properly denominated dependencies, provinces, or commonwealths.

Again, conforming to custom, we are in the habit of speaking of "the law of nations," when it is manifest there is no such thing. Law is a rule of conduct prescribed by some superior power able to enforce obedience. But sovereign states acknowledge no superior; all are equal. They recognize no common paramount authority; nor have they established any common magistracy to interpret and apply rules for the regulation of their reciprocal relations. They have no common code illustrated by judicial decisions. True, there is an established *usage* or custom in the intercourse of nations which by common consent has the moral force of law; the real meaning of which is, that there are certain forms of public *opinion* which nations, no less than individuals, cannot very well afford to disregard, although the duties thus imposed are enforced by moral sanction only. The old

Romans called this *jus enter gentes*, the French denominate it *droit des gens*, the Spaniards call it *derecho de gentes*, and we, for lack of a more specific term, call it international law. But *law* it is not; and, besides, if we admit the term at all, "law of nations" and "international law" are certainly not equivalents. The one implies an impossible condition of things, the other, though more approximately correct, would be more accurately described as international ethics or morality.

Furthermore, we are in the habit of describing what we call "international law" as "the natural law of individuals applied to nations," and when we are asked what this "natural law of individuals" may be, we reply readily that it is "the law of nature applied to moral actions," and that it consists of "rules which are common to all mankind," quite independent of the accidents of time, place, and circumstance. Now, this is little else than mere words without any definite import, for in reality there are not, and never have been, any such "rules." There is not a single, universal, fixed "rule" of human conduct which all men of all ages and countries have recognized in practice; there is no uniform moral code, written or unwritten, which peoples of all countries have even professed to obey.

But, we are told, there are certain "principles of justice, discoverable by right reason and established by usage," which *ought* to regulate the mutual relations of nations. But who shall accurately define "justice," and who shall give us an authentic standard of "right reason?" Public opinion in each sovereign state establishes a criterion of justice which rises no higher than the intellectual development or civilization of the people of that particular state; and what the people of one may consider "right reason" is often deemed wrong reason by those of another. Thus some regard all moral distinctions as merely conventional, others believe moral distinctions to have been "written in the heart of man by the finger of God." Most Christian peoples believe, or at least believe they believe, there is "a positive law, audible in conscience, which enjoins certain actions and forbids others," according to their respective suitableness or repugnance to the social nature of man. Others believe that conscience itself is merely the result of education and environment, consequently that there cannot be, in the very nature of the case, any positive moral standard. No matter how it originated, I presume that most people will agree that what we call "conscience" is nothing more than that faculty of the mind which takes cognizance of its own thoughts; that, even in the most latitudinal sense, the term can imply no more than a moral standard of action in the mind, and that this standard is always relative, that is, high or low, according to the degree of intellectual development.

We are in the habit of evading the consequences of these propositions by assuming, first, that moral distinctions have had eternal existence in the mind of the Creator, which never changes; and, second, that to Christian peoples *only* have been revealed the will of God. This would limit what we call "international law" to Europeans and their descendants on this continent; and it, moreover, assumes as a fact that, in our international relations we are governed by rules which, in their very nature, are unchangeable, which is absurb. For, reason about it as we may, we cannot get rid of the fact that our standard of morality is progressive, and therefore ever changing. There is always an advance from lower to higher conceptions of humanity and justice, and corresponding changes in public sentiment as to what is right and expedient in our international relations. The general concensus of the Christian world touching the abstract propositions of right and wrong is not what it was even one short century ago, and a century hence it will not be exactly what it is to-day. The time was when the most enlightened nations, including the one through which was derived our form of religion, spared neither age nor sex in battle. Later on, they spared non-combatants, but put all prisoners to death. Further on, the lives of prisoners were spared, but they were reduced to slavery. As civilization advanced, prisoners of war were ransomed by the payment of money or its equivalent. Finally, they were put on parole and regularly exchanged. Not many centuries ago, Christian nations went to war for the avowed purposes of conquest and selfish aggrandisement. After this, war was still held to be justifiable if waged for the declared purpose of opening new avenues of trade. Later on, war could be justified only on grounds of reasonable apprehension for national safety, or for the vindication of national honor. Perhaps the time is not very remote when Christian peoples will realize that there is a higher method of settling international disputes than that adopted by the ants and beetles, and then the principle of arbitration will be universally accepted.

Hitherto, what we call our international law has been deemed inapplicable to pagan nations and savage tribes, and in our dealings with both we have not always been governed by our own rules of justice. Our apology for this has been the assumption that such peoples are not themselves governed by the rules of justice which we acknowledge. But, if we are subject to a system of ethics which we profess to believe of divine origin, is not that, of itself, an all-sufficient reason for not departing from it in our dealings with other than professedly Christian peoples? It would seem that, if we are more than a community of hypocrites, our relations with the indigenous peoples of this continent ought to have taught us this wholesome lesson long ago.

To sum up, then, our so-called international law is but public opinion sanctioned by usage among those who call themselves Christians. But this public opinion necessarily changes with the progressive stages of intellectual development. Therefore it is not, and cannot be, a "fixed rule" of conduct in the reciprocal relations of nations. We err in calling it a "science," because our conceptions of its fundamental principles are neither clearly defined nor easily referable to known facts. And we err in limiting its application to so-called Christian nations, because we thereby contradict our professions and impair confidence in our sincerity.

BRITISH STONE CIRCLES.—II. STONEHENGE.[1]

BY A. L. LEWIS, LONDON, ENGLAND.

If the circles at Abury (or Avebury) claim the first notice on account of their great superiority in size above all others, Stonehenge naturally, and for many reasons, takes the next place to them. Stonehenge is eighteen miles south of Abury; the nearest town to it is Amesbury (three miles), but as Amesbury is not on any line of railway, Salisbury (Great Western or South Western railways) is the most convenient place from which to visit it; the distance is eight miles, six by road and two across the plain after leaving the road, and there is now no refreshment house on the way. The British entrenched hill, on which the Roman, Saxon, and Norman city stood, and which, under the title of Old Sarum, returned representatives to Parliament till 1832, at which time it was uninhabited, will attract notice, and may be visited either in going or returning.

The outer circle at Stonehenge is 100 feet in diameter, and if it were ever completed (which is a point in dispute) consisted of 30 stones, averaging 13½ feet in height; they were roughly squared and had two knobs or bosses worked on the top of each, and they were connected by smaller stones, each of which had a hole at each end, made to fit on the knobs of the upright stones on which it rested; these arrangements are found in no other circle, and are of themselves sufficient to render Stonehenge perfectly unique. One stone of this circle, still standing in its place, is shorter and slighter than the others, and this has led to doubts as to whether the outer circle were ever complete. Inside the outer circle were, first, a circle of small stones, the original number of which is uncertain, and, second, inside these five trilithons or groups of three stones, two upright and one connecting their tops, these capstones, like those of the outer circle, were kept in their places by holes fitting on knobs cut on the tops of the uprights, but while each upright of the outer circle had two knobs, and the chain of capstones was continuous, the uprights of the trilithons had but one knob each, and each pair of uprights with its capstone was separate from its neighbor; these trilithons were arranged in the form of a horseshoe, the highest (of which the uprights were 22 feet above ground) being in the centre, and the opening of the horseshoe, which is 44 feet wide, being toward the northeast. Inside this horseshoe of trilithons was a horseshoe

[1] No. I., Abury, appeared in No. 529, March 24.

of smaller stones, originally perhaps 19 in number, and from 6 to 9 feet high, the highest being in the middle, and inside these, and in front of the highest trilithon, is a flat stone, about 17 feet long and 3 wide, which is commonly called the altar stone, though, if sacrifices were ever offered there it would have been much more convenient to have had a smaller but higher altar standing upon this slab. There is a small stone lying inside the small inner horseshoe, which has two hollows and seems therefore to have been intended to rest upon two small upright stones, but no stones suitable for its support now exist, and it is possible that this stone may have stood on two small stones on the slab already mentioned, and may have been the actual altar. It has, however, been thought that it was the capstone of a small trilithon which stood in the middle of the open side of the horseshoe formed by the large trilithons, but there is no evidence as to its original position or use or as to the former existence of any small trilithon.

The smaller stones or bluestones as they are called were brought from a great distance — Devonshire, Wales, or Ireland — but the larger stones forming the outer circle and the great trilithons were obtained from the surrounding plain. Nine of the inner bluestones and nineteen of the outer ones remain, some standing and some fallen; twenty-four of the stones of the outer circle are represented by standing or fallen stones (including fragments), and six of its lintels or cross-stones are still in position; of the trilithons two are complete and the other three are more or less ruined, though all the stones of which they consisted are there, some broken, some entire.

The circles are surrounded by a slight ditch and bank, 300 feet in diameter, from which an avenue defined by earthen banks leads in a northeasterly direction for about 1800 feet, when it divides into two branches, the most northerly of which leads towards a space enclosed by earthen banks and called by Stukeley the "Cursus." Just inside the ditch and bank are two barrows, on opposite sides of the circles, and so placed that a line from one to the other passes through the centre of the circles. There are also two single stones near the inner circumference of the ditch placed like the barrows on opposite sides of the circles and so that a line from one to the other passes through the centre of the circles. At the point where the avenue joins the ditch there is a large stone lying flat, and nearly 100 feet along the avenue stands a rough stone, called the "Friar's Heel," in such a position that anyone standing on the flat stone called the "altar," already mentioned, may see the sun rise over its tip, or nearly so, on Midsummer morning, a fact which is generally verified by several people every year. It has been said that the flat stone between the Friar's Heel and the circles formerly stood upright, and hid the former from the latter, and that the coincidence as to the sunrise was therefore not intentional; but if the flat stone ever were upright the sun would have appeared to rise over it, and if neither stone existed the whole arrangement of the circles and avenue would still direct attention to the northeast or midsummer sunrise quarter.

Stonehenge has been attributed to various peoples, ranging from Atlanteans of 10,000 B.C., to Danes of the ninth century of our era, and numerous suggestions have been made as to its object. Two or three archæologists of late years have endeavored to show that it is merely the skeleton of a vast tower of dry or uncemented masonry, and the visitor must form his own idea as to the probability of this view. Burials would seem to have taken place in the centre, as bones and iron armor were dug up there in 1620, but this does not show that burial was the only or even the chief object for which the circles were constructed. Perhaps the view that best fits all the facts is that a circle or circles with avenue and outlying stones so arranged as to make it suitable for sun-worship existed here in very early times, and that long afterwards, in the dark period between the Roman rule and the Saxon domination, certain murdered Britons were buried in the circles, which were restored and re-arranged as a monument to their memory. Stonehenge, while it has much in common with the other British circles, has also so many points of difference from them, that it seems as though it must have had a special history of its own.

LETTERS TO THE EDITOR.

**** Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

On request in advance, one hundred copies of the number containing his communication will be furnished free to any correspondent.

The editor will be glad to publish any queries consonant with the character of the journal.

What is Biology?

ORIGINATING from the time of the appearance of Dr. Campbell's book[1] on biological instruction, a discussion is for the present time being held. Professor C. MacMillan opened this discussion in some very interesting articles,[2] the main feature of these being a sharp criticism of the way in which biological science has been and is taught in the colleges and universities. Mr. Francis H. Herrick[3] has tried to save the reputation of the biological departments in pleno. As the question of a clear and logical definition of the term biology meets with some of my own considerations, I should like to make a few remarks on this side of the point; the position of botanical science in the scientific institutions being merely a question of power laid in the hands of the director or professor of such institutions, I shall leave this in better hands.

It would be well, indeed, if we could get a logical definition of biology, and if we could succeed in removing from the text-books the old definition that "biology is the science of living things." Doing this, we would avoid much confusion, especially among the students — and there are many of them yet — who think that the physiological science is still a well established branch of natural science, and not merely a subdivision of a more or less heterogene "biology."

LaMarck used, first of all, the word biology, and, afterwards, from 1802 to 1822, G. R. Treviranus wrote a very remarkable book,[4] defining biology as the philosophy of living nature. Singularly, the idea of the range of living nature has, in the course of time, been limited, instead of broadened; so we see how the scientists of old times saw, in the fire, a manifestation of life. Oken, in his "System der Biologie," adopted the definition of Treviranus, while the second and third quarters of this century created physiological schools that fought against the "natural philosophers," and brought forth an experimental physiology.

When the profound thinking of Ch. Darwin (not especially of all his pupils and successors) caused a world-wide sensation, and cast new light upon natural history, the term became rather limited instead of broadened, and, in fact, from an evolutionary standpoint, we cannot, as has been done,[5] regard biology as "the science of living things." Biology has grown up with the teachings of Darwin, it is closely connected with evolutionary ideas, and, logically, appears to us in view of these teachings; therefore, we must frame our definitions in accordance therewith.

Huxley's view of the matter was taken up, and has been followed ever since, though now and then it has been modified. One of these modifications appears in a very reputable text-book,[6] biology being defined as "the science which treats of the properties of matter in the living state;" physiology, however, is "the science of action and function, essentially dynamical." I am sure that we could point out many instances of action and function that would never be classified under the heading of physiology or even biology, nay, "*general* biology." On the other hand, I doubt if physiological science is really characterized by the word dynamical; in other words, if "physiological action and function" necessarily presupposes something "dynamical."

[1] John P. Campbell, "Biological Teaching in the Colleges of the United States," Bureau of Education, Circular of Information, No. 9, 1891.

[2] Botanical Gaz., xvi., p. 301, 1892 (see also pp. 260 and 336). Science, April 7, 1893, p. 184.

[3] Science, April 21, 1893, p. 220.

[4] Biologie oder Philosophie der lebenden Natur., Vol. 1-6, 1802-1822.

[5] Huxley, "On the Study of Biology (Lectures on Evolution)." See "Humboldt Library," No. 36, 1882, p. 37.

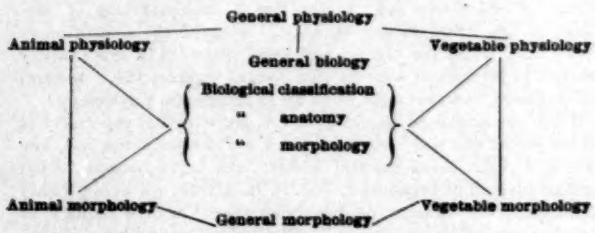
[6] Sedgwick and Wilson, "General Biology," New York, 1886, pp. 7-9.

We have had for several hundred years the term physiology, which is the science of the life-phenomena.[1] There is no reason why we should not retain this name, and use it as it has been used ever since the revival of science in the sixteenth century. Biology is of later origin, it was born with evolution, and it is merely a branch of the all-embracing physiology. Biology does not consist of the entire sum of life-phenomena; it is the branch of physiology which treats of *the mutual relationship of the forms of organized matter, especially in view of the theories of adaptations and of natural selection.*

I wish to confine my remarks as far as possible to vegetable biology, and here I shall invite your attention to a very important paper by F. Delpino,[2] who regards biology as the main basis of Darwinism, and points out its importance for the theories of plant metamorphosis. With reference to the latter, we find that Warming[3] will admit only the "definition of metamorphosis" into the biology. Goebel[4] explains the state of affairs very logically in these words: "Biology regards the parts of the plants as if they were not limbs (in morphological sense), but organs, or tools," and thus he mentions one feature of biological investigation, namely, the study of correlation.

If physiology is placed at the head of natural science, and biology in its proper place as a branch thereof, we shall be able to see more distinctly how to reach the ideal, namely, the comparative physiology of animals and plants, for which so much material has been accumulated that we are able to grasp many important features of *life in general.* I have repeatedly[5] referred to this fact, but it will be admitted that the *fundamental definitions* must be logical and not ridiculous.

How biology, in the true and limited sense, branches out into the other departments of botany, I have shown in the following table. We have two features of living things: form and function, and, accordingly, the morphology and the physiology. The table shows



how we get a biological classification, or a comparative systematic botany, or zoölogy. Biological morphology is practically a morphology which deals with adaptations of the different forms to certain ends and comparatively regarded. Biological anatomy is teaching the structural adaptations in animals and plants from a comparative standpoint.

To apply biological characters and features to the systematic part of either zoölogy or botany will tend to make the registration of species and forms more valuable to physiology.

Probably it seems trifling to write quite elaborately about a question of definition. If, however, our fundamental definitions shall be not merely *adaptations for the extension of private power and influence*, we must consider them well. This is not only a question of logical consideration, but of scientific principles.

J. Christian Bay.

Missouri Botanical Garden, April 27.

[1] See J. von Sachs, "Vorlesungen über Pflanzenphysiologie," 1887, p. 3.

[2] Fondamenti di Biologia vegetale, I.; Prolegomeni (Revista di Filosofia scientifica, Milano., I., 1880, No. 1, pp. 58-90). See Botanisches Centralblatt, vol. ix., 1882, pp. 333-335.

[3] Warming, in Meddelelser fra den bot. Forening i Kjoebenhavn, I., 192.

[4] Pflanzenbiologische Schilderungen, I., 1889, Introduction.

[5] See Science, March 24, 1893, p. 162; Bot. Gazette, xvii., 1893, p. 105; Biologisches Centralblatt, xiii., 1893, p. 38.

Epidermic Forms of Mental or Nervous Diseases or Disorders.

It is very desirable that certain data should be gathered on "epidermic forms of mental or nervous diseases or disorders.' As an example of what is meant, I would instance "The Children's Crusade," which occurred in Europe; the persecution of certain individuals supposed to be possessed of witches in New England, and chorea, or St. Vitus's dance, occurring among school children; panic is another form very common, especially at the present day.

Could any of the readers of *Science* furnish me with any information of occurrences which have come under their notice or which they may have read about? They are certainly very common, for one reads of them very often in the daily papers. If some of your "live" readers would consider this subject seriously, and send so full reports as possible, they would not only be doing a personal favor, but would certainly be contributing toward an interesting and important collection of scientific facts.

James Wood, M.D.

167 St. John's Place, Brooklyn, N.Y.

Color of Flowers.

I have just seen Miss Neal's question in your issue of March 31, 1893, as to how to preserve the colors of flowers when pressing them. If some of your readers have not already sent a better recipe, the following may be found useful.

Immerse the stem of the fresh plant in a solution of 31 grains of alum, 4 of nitre, and 186 of water for a day or two, until the liquid is absorbed, then press the plant in the usual way, sift some dry sand over the flower, and submit to a gentle heat for about twenty hours.

I have found this process pretty successful. A. B Steele.

Edinburgh, Scotland, April 28.

The Aurora.

In my contribution to *Science*, April 7, on the above subject, no mention was made (as required by Dr. Veeder in his reply in the issue of April 28) of a particular instance of want of coincidence between auroral display and solar disturbance at the eastern limb, for the following reasons: First, because I have, so far, considered each phenomenon as being dissociated, or rather not connected in the manner stated; second, because I do *not* think it possible to point out such a want of coincidence with the very liberal limits of time evidently comprised in the term "eastern limb" by the advocates of this theory; and, third, amidst the bewildering number of instances, which must occur between even dissociated phenomena of such frequent occurrence, even when the limit spoken of is of reasonably brief duration, it is possible (most probable) that coincidence will be mistaken for cause. That this coincidence is not so great as claimed, seemed to me to be indicated by the results mentioned as obtained by Greenwich, as also by the same conclusion arrived at by Professor Ricco, as mentioned by Dr. Veeder; surely this is a fair assumption to make, if discussion of the same or similar records give results so widely different?

Personally, I do not wish to take any part in this discussion. Dr. Veeder's theory has constantly appeared in the press and by pamphlet without any attempted refutation; believing it to be founded on false premises, I have felt called upon to act as censor, failing any one else.

Granted a very large number of coincidences between auroral displays and the position of a disturbed area at the eastern limb of the sun; if Dr. Veeder will place a limit of, say, twenty-four hours for the term "eastern limb," and consider occurrences beyond this as not being coincidences, I believe he will find that there are as many auroras (I should be inclined with this limit to say, very many more) which occur without this particular solar source of energy as with it. Again, allowing *any* interpretation of the term "eastern limb," and, applying the same interpretation rigorously throughout, I think it will be found that the proportion of coincidences will increase from the minimum sunspot period to the maximum, and that this coincidence will vary

directly as the number of sun-spots visible. Now, if there were an intimate connection between the two classes of phenomena, the appearance of an area of great solar disturbance at the eastern limb, as is occasionally the case at the time of a minimum, should give *very* marked auroral displays, whereas it is quite certain that the coincidence is not so marked at these times (where the element of "chance" is reduced) as at the time of a maximum; is this not so?

Auroras are, or are not, an effect of sun-spots on the sun's eastern limb. I spent fourteen months in Hudson's Strait, and, to my knowledge, during the auroral season from 50 to 75 per cent of our clear nights (and clear nights were a peculiarity of the latitude in winter) had auroral displays. Assuming two such solar areas as required constantly on the sun, and representing the term "eastern limb" by twenty-four hours, we have a vastly larger number of auroras unaccounted for than this theory accounts for.

Quoting from Dr. Veeder's letter to *Science*, April 28, he says: "When, however, this area was at the eastern limb, from Jan. 7–11, *although it had not yet developed spots*, and was the seat of brilliant faculæ only, . . . great magnetic storms" were "in progress and auroras . . . reported in high latitudes."

I never saw, nor do I expect to see, the eastern or western limb of the sun when faculæ are visible to ordinary powers, when they were not more distinctly "brilliant" there than elsewhere. If this condition can be taken as a fulfilment of this theory, it is evident that the theory is beyond argument.

This quotation furnishes the required instance "in which an aurora appeared in the absence of *well-defined* solar conditions," for, according to the evidence supplied, "a great magnetic storm" was in progress from Jan. 7–11, whereas I feel certain that Dr. Veeder cannot claim that an area represented by *five* days' solar rotation (Jan. 7–11) could possess (in fact, his words show it did *not* possess) *well-defined* solar conditions of the nature required.

Sun-spots[1] have been a special object of study at this observatory since its institution. It is safe to say that something is known of their nature and origin, but that it is as nothing to that which remains to be investigated. It is possible to allow fanciful attributes to this little-known agency, which will account for any theory we may be pleased to conceive, but, treated in accordance with any known dynamical law, there seems to be no way of accounting for the peculiar action of this force, which is not equally applicable to its position at the western limb. It seems evident, from the nature of a sun-spot's formation, that the force employed is exerted in a vertical direction; it would be reasonable to expect that the resulting maximum effect should be evident, if at all, in the same direction; not horizontally, as this theory requires.

Assuming the solar force to be an "electro-magnetic" one, any resulting auroral development should bear a fixed relation to the line joining the source of energy with the earth's centre and the plane of rotation of the earth. If this is a fact, it is quite evident that points widely differing in longitude on the earth's surface will experience similar effects, as the earth's diurnal motion brings them successively under this influence, after a time-interval almost infinitely less than that represented by the difference of longitude of the two points considered. No one will surely claim that this is even approximately the case.

Again, "cosmical dust and debris" is not conclusively present in the "zodiacal light." Even accounting for the origin of the zodiacal light in this way, it is observationally evident that the rest of interplanetary space is not so filled, for this light is only visible as an appendage to the sun, in certain fixed directions; elsewhere the absence of the light proves that this "dust and debris" is not symmetrically disposed about the sun. Admitting, for the sake of argument, that interplanetary space was filled with this dust and debris, the lapsed æons of planetary existence with the countless orbital revolutions of the planets themselves must have swept out, as the masses of the planets must have aggregated to themselves, the last vestige of such dust and debris, leaving vast intervals without this assumed conducting material.

[1] "Sun-Spots: Their Maximum and Minimum Periods and Zones of Greatest Frequency." Read before the Royal Astronomical Society, April 13, 1892.

I should be pleased, and I think it would be a matter of more than personal interest, if Dr. Veeder has the time, in what I know to be a very busy life (setting the "limit" I have suggested), if he would, from out the fund of information in his possession, see how far the element of "chance" enters into this question, not admitting too much of the suppositional when sun-spots fail at the required period by the substitution of "faculæ," and at the same time show a comparison of coincidences through a semi-period, at least, of solar activity.

W. A. ASHE.

The Quebec Observatory, May 6.

BOOK-REVIEWS.

The Earth's History. An Introduction to Modern Geology. By R. D. ROBERTS. New York, Chas. Scribner's Sons. 1893. Maps and illustrations. 270 p. 12°. $1.50.

THIS volume is one of a series now being published in England by Murray and in this country by the Scribners, as an outcome of the popular University Extension movement. The prospectus states that "the aim of these manuals is to educate rather than inform. In their preparation, details will be avoided except when they illustrate the working of general laws and the development of general principles; while the historical evolution of both the literary and scientific subjects as well as their philosophical significance will be kept in view."

The author of the present volume has been successful in carrying out this plan, for without being detailed he presents the broader aspects of the science in a familiar and pleasing manner. In the chapter on the "Agents of Destruction," he refers particularly to the Grand Cañon region, where the phenomena of denudation are shown on such a magnificent scale. This is followed by chapters on the extent of the destructive operations in Nature, and these, in turn, by other chapters on the construction of land. The constructive agents are grouped under the three heads of deposition, movements of the crust, and addition by extrusion from the interior. There are interesting accounts of shallow-water deposition, of calcareous deposits, such as coral reefs, and of deep-sea deposits. The author does not commit himself in regard to the origin of atolls, referring to Darwin's theory of subsidence, but not discussing others that have been advanced. An interesting account is given of the formation of Monte Nuovo in 1538 and of the destruction of Krakatoa in 1883.

The last part is devoted to the "Evolution of Land Areas," and we have here the application to geological phenomena of the principles enunciated in the first parts. Two chapters deal with the evolution of the British Islands. Altogether the volume gives an excellent exposition of geological phenomena and must serve as a useful compend to all who desire a knowledge of the principles without having to wade through a mass of details concerning the subject. For these details other volumes must be consulted.

JOSEPH F. JAMES.

Washington, D. C., May 8.

Public Health Problems. By JOHN F. J. SYKES. The Contemporary Science Series. New York, Charles Scribner's Sons. 8°.

THE multiplication of books relating to public health may perhaps in itself be encouraging, but the fact that the quality in no way keeps pace with the quantity is quite the reverse. The book before us covers a wide field — from "heredity" to "dwelling-houses" — but conveys, whether rightly or wrongly, the impression of being in the main the result of a "cram." The chapter on heredity, for example, opens with this remarkable statement, "The Darwinian theory of natural selection has given prominence to two schools of evolutionists, the one attributing evolution solely to selection, and the other, whilst not denying the effects of selection, valuing — perhaps over-valuing — the effects of heredity" (p. 8). If the reader be fairly conversant with modern biological literature and be in a somewhat cynical mood he will at least derive some amusement from the rest of that chapter.

It is perhaps unfair to single out the chapter on heredity for especial criticism since the subject is rather remote from the author's main theme. We regret, however, to be obliged to point

to the fact that the succeeding chapters are sprinkled with far too many misstatements and that the whole book is marred by a loose and slovenly style. We do not wish to imply that the book does not contain many valuable and interesting facts, but the general lack of precision of statement is painfully evident in a passage like this: "It has been hitherto [sic] held that putrefaction was a chemical action only, but recent researches have shown that numberless microbes are concerned in the process, and without these micro-organisms organic bodies retain their form" (p. 69).

Debatable questions are dismissed in a rather summary fashion, e. g., "There can be no doubt whatever that sewer gas may produce sore throat, diarrhœa, and typhoid fever. . . ." (p. 65). "the germs of disease may be easily carried into the air from refuse and fæcal accumulations" (p. 75).

As for the style, "Koch demonstrated the presence of cholera bacilli in the water of Indian ponds or tanks, probably harboring and multiplying in the banks" (p. 76.). "However urgent those specially familiar with the deteriorating influences at work may regard the remedies applicable, yet they can never secure their adoption without the consensus of the opinions of others" (p. 355).

The author in a measure, however, disarms criticism by his unimpeachable statement in the conclusion (p. 344), "Errors of omission and of commission may be readily found in all human work . . ."

Report on the Brown Coal and Lignite of Texas. By E. T. Dumble, State Geologist. Austin. 1892. 248 p. Plates. 8°.

This volume is one of the numerous ones that have recently appeared on the work of the survey. It contains a very full account of the origin, formation, and composition of the Brown coal and of its use as fuel. Many details are given of the geology of these deposits in Texas, and comparisons are made between these and the European lignites. A strong argument is made for the use of the Brown coals in Texas, and the results of the investigations made by the author in Europe and in Texas may be summed up about as follows:—

Brown coal and lignite of good quality are capable of replacing bituminous coal for all household, industrial, and metallurgical processes.

Texas has an abundant supply which is so situated as to permit its being mined and delivered for use at a far less cost than bituminous coal.

The raw coal can be used in stoves and grates, under locomotive boilers, in iron smelting, lime burning, etc. It may be used for the manufacture of gas for lighting or heating. It can be made into artificial fuel by "briquetting" with coal-tar, pitch, etc., and then used like ordinary coal. Certain varieties, if charred, will form a coke with coking coal and coal-tar pitch, which can be used for locomotive engines and other similar purposes.

These facts are of great importance to a country like Texas, where wood is practically absent, and where the ordinary soft coals and anthracite are nearly unknown. There seems no reason why similar deposits of lignite in other States west of the Mississippi River should not be utilized. Joseph F. James.

Manual of Machine Drawing and Design. By D. A. Low and A. W. Bevis. London and New York, Longmans, Green & Co. 1893. VI. 375 p. 8°. $2.50.

This excellent work is designed for the use of engineers and their apprentices, and for students in technical schools, and is admirably adapted to its purpose. It is more a drawing-room

CALENDAR OF SOCIETIES.

Anthropological Society, Washington.

May 16.—Annual address by the president of the society, Dr. James C. Welling, The Last Town Election in Pompeii: An Archæological Study of Roman Municipal Government based on Pompeian Wall Inscriptions.

Society of Natural History, Boston.

May 17.—Clarence J. Blake, Out of Darkness into Light; or, The Education of a Blind Deaf-Mute. Mr. Anagnos kindly consented to allow Miss Thayer of the Kindergarten for the Blind, and her pupil, Willie Robin, to be present.

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TO VOLUME XVIII OF
SCIENCE
is in preparation, and will be issued at an early date.

text-book than one for class-room use, such as Unwin's. It includes less mathematical discussion than samples of good designing, graphically presented. It is full of excellent "dimensioned" illustrations of a great variety of machinery, and especially of machine-tools and steam-engines. It gives a large number of rules and tables of proportions of parts of machinery taken from the standard treatises and from the note-books of skilled designers. In many cases the methods of computing sizes and proportions are given. The drawings have all been prepared from working drawings, and especially with a view to their use for this purpose. Standard and successful practice is thus laid before the young student, or practitioner; and the art of proportioning is thus not only acquired, but the novice is, at the same time, made familiar with the best designs of his seniors. A combination of this work with that of Professor Unwin would seem likely to make an ideal course; the one being used in the drawing-room, the other in the class-room in conjunction with lectures. For the ambitious apprentice, no better plan could be recommended than a similar course of private reading and practice.

The Philosophy of Individuality, or the One and the Many. By ANTOINETTE BROWN BLACKWELL. New York, G. P. Putnam's Sons. $3.

THIS work is a new attempt to solve the problem of the universe. It is by no means easy reading, the style being at once verbose and obscure, and the same thought is often repeated again and again, without ever being made clear. The fundamental doctrine of the book is a new theory of matter, namely, that matter is not a substance at all, but merely a complex of motions; or, in the author's own words, that "matter is literally composed of aggregated and coöperative modes of motion." Even an atom is regarded as nothing but a combination of balanced and correlated motions: "Our atom of matter, then, is a unit of motions with innate energy enough to achieve vastly more than has yet been required of it by physical evolution." This view is expounded and illustrated through several chapters, and the endeavor is made to show how the theory applies to what we commonly call substances, and to distinguish between these "complexes of motion" and the "free motions" of heat, light, electricity, and gravitation. The theory is admitted to be nothing but a hypothesis, and we fear that it will always remain so.

Passing now from the realm of matter to that of mind, the author presents a theory of mind and consciousness similar to that of Herbert Spencer, that mind and matter are merely two aspects of one underlying reality. It differs, however, quite radically from Spencer's view in regarding life and consciousness as attributes not of an organized body only, but of each individual atom: "The rhythmic atom is *alive* with the high possibilities of ever-growing sensibility and actual knowledge." The objection that there is no evidence of life or feeling in inorganic matter, Mrs. Blackwell endeavors to meet by the theory of "potential mind" and "nascent feeling," phrases which are made to do duty instead of arguments and proofs. The grand difficulty with such a theory is to account for personality; for, if every atom is sentient by itself, it would seem that I must have as many minds as there are atoms in my body, and Mrs. Blackwell is by no means successful in removing the difficulty. "We assume," she says, "that the one commanding ego in each higher organism is exclusively but one individual unity!" but, notwithstanding her exclamation point, there is no warrant in her theory for such an assumption

Such are the fundamental doctrines on which the author seeks to found a rational theology and a belief in the immortality of the soul, but we find little in her arguments that is convincing or satisfactory. The whole theory is hypothetical; and, while we recognize the earnest purpose of her book, we cannot think that she has added anything important to our knowledge of nature or of man.

QUERY.

Can any reader of *Science* cite a case of lightning stroke in which the dissipation of a small conductor (one-sixteenth of an inch in diameter, say,) has failed to protect between two horizontal planes passing through its upper and lower ends respectively? Plenty of cases have been found which show that when the conductor is dissipated the building is not injured to the extent explained (for many of these see volumes of Philosophical Transactions at the time when lightning was attracting the attention of the Royal Society), but not an exception is yet known, although this query has been published far and wide among electricians.

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